

Dr. Siemens has, in his paper, further suggested that solar radiation may effect the dissociation in interstellar space of the compounds of oxygen with carbon and with hydrogen, so that these elements may reach the sun in an uncombined state, and there be burned. He would thus make the sun not only a compressing-engine, but a furnace. While such a dissociation in outer space is not impossible, it is to be said that a preliminary decomposition, followed by reunion in the solar sphere, would in no way augment the ultimate calorific effect of compression there. The elements in the act of dissociation in space would absorb just as much radiant energy as would be set free by their subsequent combination, so that, whether the solar radiations are expended in heating or in dissociating the diffused matter, the final result in the sun would be the same. It may be further remarked, that from what we know of solar chemistry, dissociation of aqueous vapour and of carbonic dioxide is more likely to take place in the sun itself than in the cold regions of outer space.

While, therefore, his suggested addition to the hypothesis seems, if not untenable, unnecessary, we are grateful to Dr. Siemens for again bringing before us the grand conception which dawned upon the mind of Newton, but has found its fuller expression in our own day, and, as I have endeavoured to show in the papers already noticed, gives us the elements of a rational Physiology of the Universe.

T. STERRY HUNT

Montreal, Canada, April 3

THE two preceding letters by American men of science of well-known position, grant one of the three postulates upon which I grounded my solar plan, that of space filled with attenuated matter; they do not object to the second, and all-important one of the equatorial outflowing current; but they call in question the necessity of the third, that of dissociation of attenuated matter in space by means of arrested solar energy. Both my critics think dissociation in space unnecessary for the maintenance of solar energy, or as Dr. Sterry Hunt very clearly puts it: "Whether the solar radiations are expended in *heating* or in *dissociating* the diffused matter, the final result in the sun would be the same."

I would be disposed to agree with this dictum if taken as an abstract proposition, but I do not think that my critics can have subjected their view to calculation, the keystone without which the arch of speculation cannot be considered as secure. We know by experimental evidence that stellar space, and the matter filling it, are intensely cold, as proved by the winter-temperature of the polar regions; moreover water exposed even in the tropics to free radiation while insulated from the warm earth, freezes to a considerable thickness during a single night.

Let us suppose that the attenuated matter in space has a temperature of 160° on the absolute scale (being 114° below the freezing-point of water), and that it is 3000 times more rarefied than when it reaches by adiabatic comparison the solar photosphere. The rise of temperature due to this compression must be according to Rankine's well-known formula—

$$\tau_2 = \tau_1 \left(\frac{p_2}{p_1} \right)^{\frac{\gamma - 1}{\gamma}} = 29 = 1632^\circ \text{ absolute,}$$

and this would make the solar photosphere 1358° on the Centigrade scale; this temperature is quite inadequate to produce the solar luminosity, which must require one equalling, though probably not exceeding that of the electric arc.

But assuming a compression of the attenuated atmosphere up to the photospheric density (which according to most authorities does not exceed terrestrial atmospheric density), there would still remain the predicament that although a higher maximum temperature could be reached by compression, very little of the heat due to it could be spared for the purpose of radiation, without sacrificing the possibility of disposing of the refrigerated gases again into space. The refrigerated gases would obey the law of solar gravitation to a much greater extent than the heated incoming gases, and would certainly not pass away into space, unless acted upon by a considerable extraneous force. The mere passage of the solar orb through space at a majestic pace which does not exceed one quarter of our orbital velocity, could not possibly produce such a result, and ever the fan action advocated in my paper would fail to work in opposition to a large determining influence of solar gravitation.

These conditions are entirely changed if we assume, in addition to adiabatic compression and re-expansion, a further source

of heat such as is produced in combustion. One pound of hydrogen develops in burning about 60,000 heat units, and one pound of marsh gas 24,000 heat units; in my article upon this subject, published in the April number of the *Nineteenth Century*, I showed that if only one-twentieth portion of the gases streaming in upon the polar surfaces at the pace of 100 feet a second were combustible gases, they could produce an amount of heat more than sufficient to account for the entire solar radiation as determined by Herschel and Pouillet.

There is no reason for supposing that the instreaming gases would penetrate beyond the solar photosphere; they would flash into combustion whenever their temperature by adiabatic compression had reached the limit of spontaneous ignition without the presence of an igniting solid, a point which, if determined experimentally, would give a clue to the real vapour density of the photosphere; and after reaching the point of dissociation, combustion would continue in the measure of the abstraction of heat by radiation, thus producing a vast accumulation of igneous matter of comparatively low density. This would flow on, in the manner of a floating body, above the denser gases or vapours forming part of the permanent body of the sun, towards the equatorial regions, whence it would be propelled into space at a temperature exceeding to some extent that of the inflowing gases after compression, but before combustion, thus aiding, instead of retarding the supposed solar fan action.

The fan-action itself would be produced, no doubt, at the expense of solar rotation; but, in order to appreciate this retarding influence at its true value, it must be borne in mind that the flow of gases once established has only to be changed in direction; the velocity acquired by the inflowing gases is simply transferred to the outflowing current diminished by an amount of rotative force sufficient to cover frictional retardation. The very interesting leading article in last week's *NATURE*, regarding the solar observations in America, during the last eclipses, now published for the first time, furnishes an unexpected and most striking corroboration of the solar fan-action which I had ventured to put forward as a necessary consequence of solar rotation in space filled with attenuated matter.

I am well aware that my paper read before the Royal Society does scant justice to those who have devoted much time and ingenuity to the subject of solar physics, and that, moreover, many points of considerable interest connected with the views I advocate have been indicated only, instead of having been fully developed; but, on the whole, I thought it was better to present my views in mere outline before an audience well acquainted with our present information regarding solar physics, and with only half an hour's time at their disposal.

The elaboration of such a subject would necessitate the writing of a book rather than of a paper, and perhaps Dr. Sterry Hunt, who has already done so much to elucidate our present knowledge of solar physics, may be induced to extend his labours in this direction.

C. W. SIEMENS

12, Queen Anne's Gate, Westminster, April 26

Silurian Fossils in the North-West Highlands

My friend, Mr. Hudleston, in his letter on the Silurian fossils in the North-western Highlands, states very clearly a point which at the present time is of the highest importance to all students of the metamorphic rocks. If it can be proved that the Durness limestone, which contains undoubted lower Silurian fossils, is identical with the series in Western Sutherland and Ross, which Mr. Hudleston terms the quartz-dolomitic, then the so-called "Newer Gneiss" must be more recent than it, and thus must be a metamorphosed representative of some part of the Silurian series. This would prove that very great regional metamorphism has taken place in the latter half of the Palæozoic period; and that its mineral condition will not aid us materially in determining the age of a rock which has once been stratified.

But is this identity proved; and is it certain that the Durness limestone is more ancient than the Newer Gneiss series? I have not myself had the opportunity of investigating the Durness region, though I have examined several specimens of its limestone; and from the condition of these and my knowledge of parallel cases, and of metamorphic rocks in general, do not hesitate to say that I should require very clear stratigraphical evidence before I could believe the Durness limestone to underlie the "Newer Gneiss." The former is no more metamorphic than are several of the Palæozoic limestones; the latter is always considerably, sometimes rather highly, metamorphosed. But in

the quartzo-dolomitic series the amount of metamorphism, though the materials are not favourable for its production, is considerable; and the rock has a general resemblance to some of the impure calcareous bands which are incorporated with true schists in the Alps.

Further, although our knowledge does not at present enable us to speak dogmatically on this point, the weight of evidence is, in my opinion, strongly against the probability of the Newer Gneiss series being altered Silurian rock. I would even go so far as to say that it is such as to throw the *onus probandi* on those who assert its (comparatively) modern date. For five or six years I have been working—I trust without prejudice—at the question of the age of metamorphic rocks, during which time I have visited typical districts in Cornwall, Wales, Scotland, and the Alps; and in every case have been driven to the same conclusion, namely, that wherever extensive regional metamorphism exists, the antiquity of the rocks is very great, so that they are probably anterior to the Cambrian period. I fully expect that when the Durness region is closely scrutinised, it will be found that this fossiliferous limestone is faulted down against the metamorphic series, exactly (for instance) as the so-called Devonian rocks of the Lizard are faulted against the “hornblende schists” of that district, and are a remnant, thus preserved, of a more modern and wide-spread series. Any geologist who would settle this point for us would be entitled to our gratitude, but to do it will require no ordinary conjunction of qualifications; for he must be a practised microscopist, a skilled worker in the field, and a man who cares for truth more than for the traditions of an office, or even his own preconceived opinions.

23, Denning Road, Hampstead

T. G. BONNEY

WITH regard to Mr. Hudleston's letter on the above subject, published in NATURE (vol. xxv. p. 582), I am glad to say that I am still alive, and able to give a part, at least, of the desired evidence for connecting the Durness limestone with the rocks of Assynt and Erribol.

In the year 1858 I accompanied Sir Roderick Murchison, while on a geological tour in Sutherland. During our stay at Inchadamff, one of our excursions led us together up the River Traligill. Opposite the place where the springs issue from the miniature limestone caverns, about two miles above the bridge, I espied the fossils in dispute—“orthoceratites”—partially weathered out of the dolomitic limestone from which the stream issues. So overjoyed was I, that I called Sir Roderick to my side by shouting “Eureka,” as I was a little in advance of him, pointed out the fossils *in situ*, and after hammering them out of their bed, handed them to him. The circumstances of the achievement are indelibly impressed on my memory. As I only saw these fossils in the field, I am not able to tell to what species they belonged; but there can be no doubt of their nature, as in my attempt to hammer them out of the rock, one of them was broken in such a manner as to expose the septa and the sphuncle.

On a subsequent visit which I made to Sutherland, I had the good fortune to see the specimen of *Orthoceras* (*Cameroceras*) *Brongniartii* alluded to by Mr. Hudleston as “having been found in the upper quartz-rock of Erribol.” It was in the possession of the finder, the late Mr. Clark, of Erribol House, who kindly allowed me to examine it. Mr. Clark accompanied me to the place, and pointed out the exact spot where he got the specimen—a little to the north-east of Erribol House.

CHAS. W. PEACH

30, Haddington Place, Edinburgh, April 24

The Magnetic Storms

THE magnetographs at the Kew Observatory were a little disturbed from about 11 p.m. of the 13th inst. to 7 p.m. of the 14th inst. During the 15th they were quiet, and remained so up to 11.45 p.m. of the 16th, when the disturbance began by an increase of the declination, an augmentation of the horizontal force, and a diminution of the vertical force. The movements of the declinometer became gradually more rapid after 2 a.m. on the 17th, whilst its oscillations extended farther and farther from its normal position principally in the direction of increased westerly declination.

From 4.30 to 9 a.m. the horizontal force had diminished so much that the trace frequently passed off the paper and the register was lost for a while. At this time the force must have been more than .05 mm.mgrs. below its average value.

The minimum of vertical force occurred at 5.55 a.m., when it was about 0.07 units too low.

From 10 a.m. to noon of the 17th the motion of the declinometer was small, whilst the components of magnetic force were rapidly increasing in intensity, until at 0.15 p.m. both traces left the photographic sheet in the direction of augmented force; at this time the declination needle merely oscillated rapidly about its ordinary position.

The horizontal force instrument recommenced to record about 2 p.m., and the vertical force about 2.45 p.m.; afterwards the movements of all three gradually diminished, and at about 8 p.m. the disturbance had died out.

During the 18th and 19th the magnets remained unaffected, but at 3.45 a.m. of the 20th a second disturbance set in, commencing with a rapid increase of declination, the first swing of the magnet carrying it nearly a degree to the westward, whence it returned at 4.30 a.m. Its mean position was reached at 6 a.m., and then its oscillations became very rapid, and continued so until 2 p.m., after which hour they became less; but the effect of one disturbance lasted until 7.30 a.m. of the 21st.

Both forces were also simultaneously disturbed, but their movements were much more limited than on Monday, the extremes being in the horizontal .04 mm.mgrs., and in the vertical 0.3 mm.mgrs. only.

G. M. WHIPPLE

Kew Observatory, April 24

Colour Perception

WHILE working at dry-plate photography in a ruby light, I noticed that when any light-coloured article, such as the hand, was rapidly moved, it appeared of a brilliant greenish-blue, in which blue predominated, while, when slowly moved, it appeared of the same colour as the other objects in the room. Seeking for an explanation, led me to recognise a new fact about colour perception which may be of interest to your readers. The reason of the hand appearing blue when in rapid motion was because the continual use of the red light had fatigued that part of the retina responsive to it, and the light reflected from the hand impinging for a very short time on the retina, was not strong enough to excite the sensation of red, but was quite sufficient for blue, the nerves responding to this colour having been rendered acutely sensitive by complete rest. To test this hypothesis, I obtained some dark blue glass and applied it to the window of the dark room, removing the red. On repeating the experiment, the eye with its blue sense exhausted, saw rapidly-moving objects reddish. Now from this it is clear that it takes a longer time to cause a sensation in an exhausted than in a fresh organ. It also gives a direct proof of Helmholtz's suggestion, “that actual coloured light does not produce sensations of absolutely pure colour; that red, for instance, even when completely freed from all admixture of white light, still does not excite those nervous fibres alone which are sensitive to impressions of red, but also to a very slight degree those which are sensitive to green, and perhaps to a still smaller extent those which are sensitive to violet rays” (“Popular Scientific Lectures,” first series, p. 223). These observations have led me to an explanation of a very curious phenomenon brought under my notice by my friend, Mr. Napier Smith. When discs of paper on which black spaces have been marked, so that on rotation the eye receives impressions of black and white too rapidly to notice the pattern, but too slowly to combine into a neutral gray, the rotating card appears to be distinctly coloured, especially when it is looked at without keen attention, or as we may say passively. All colours may be seen, but red and blue were the most distinct to me. I at first thought that the colour might arise out of the paper and ink, the former being perhaps tinted with blue to whiten it in manufacture, and the latter probably a dark brown; but on looking several times at the rotating discs, and acquiring the power of looking passively the intensity of the colours could not be so accounted for. The true explanation is found, I believe, in the fact that the different colour organs require longer or shorter periods of excitation before responding to the stimulus, and that those which require the longest periods also retain the sensation longest. I have only made very rough trials, but they point to the fact that the eye responds quickest to red, so that the most rapid alternation will appear reddish, a little slower green will come in, and cause some indescribable colours, such as are seen in the polariscope, and lastly, when green and red are about equal, and producing white, blue will be seen. The blue is best seen with a slow